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Job loss, firm-level heterogeneity and mortality: Evidence from administrative data

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ABSTRACT

This paper estimates the effect of job loss on mortality for older male workers with a strong labor force attachment. Using Dutch administrative data, we find that job loss due to firm closure increased the probability of death within five years by a sizable 0.60 percentage points. Importantly, this effect is estimated using a model that controls for firm-level worker characteristics, such as lagged firm-level annual average mortality rates. On the mechanism driving the effect of job loss on mortality, we provide evidence for an effect running through stress and changes in life style.

1. Introduction

Job loss is a frequent event that many individuals experience in their lives. From January 2013 through December 2015, there were about 3.2 million workers displaced from jobs they had held for at least 3 years in the United States alone (Bureau of Labor Statistics, 2016). First-order effects on workers' economic circumstances may be due to the associated income loss that can have long-lasting, permanent effects (Jacobson et al., 1993; Couch and Placzek, 2010). Job loss, and in particular, layoffs, can be associated with strong effects on people's lives in general and health in particular, through a variety of channels. Workers may get surprised by a sudden career disruption, experience subsequent job instability (Stevens, 1997), have difficulties coping with the change and experience detrimental effects on their life style (Deb et al., 2011; Falba et al., 2005), financial situation (Jacobson et al., 1993; Couch and Placzek, 2010), physical health (Black et al., 2015) and mental health (Kuhn et al., 2009). In the extreme, involuntary job loss may result in death (Sullivan and Von Wachter, 2009; Eliason and Storrie, 2009a; Browning and Heinesen, 2012).[1] We reinvestigate the effect of job loss on mortality, using administrative data from the Netherlands. We find evidence of a sizable positive effect.

Estimating the effect of job loss on mortality is challenging, as job loss can occur for many reasons and is likely to be endogenous to health and mortality.[2] Previous studies typically try to handle the endogeneity issue by considering specific causes of job loss as treatments: job loss due to firm closure (Eliason and Storrie, 2009a; Browning and Heinesen, 2012; Michaud et al., 2016), job loss due to firm bankruptcy (Keefe et al., 2002) and job loss in firms experiencing large firm-level employment declines (Sullivan and Von Wachter, 2009). Control groups consist of workers employed in continuing firms, or workers in firms unaffected by large workforce reductions, respectively. The rationale for drawing these specific samples is that individual worker health behavior, lifestyle, and death probability, are independent of individual job loss in the treatment group. The approach potentially ignores selective work-

[1] The general economic circumstances causing job loss may affect health and mortality as well. Ruhm (2000), for instance, finds that mortality is procyclical.

[2] The effect of job loss on health and mortality is studied in other strands of literature as well, such as epidemiology (e.g. Gallo et al., 2006; Eliason and Storrie, 2009b).

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force dynamics, however. In particular, it requires the absence of a priori differences in workers' health outcomes between treatment and control group. Workers employed in closing firms and firms experiencing large employment declines may have poorer health than those employed in firms that do not close or do not shrink. Possible pre-existing differences in firm-level worker health measures and mortality rates may exist due to selective hiring or due to non-random worker outflow prior to treatment. Not controlling for potential pre-existing differences in firm-level worker health and mortality rates may lead to upward bias in the estimate of the effect of job loss on mortality. We aim to avoid such bias by explicitly controlling for pre-existing differences in firm-level worker mortality rates and health and other firm-level worker characteristics. To our knowledge, this is the first paper in the literature that follows this approach.

Earlier studies typically find a positive effect of job loss on mortality (Sullivan and Von Wachter, 2009; Eliason and Storrie, 2009a; Browning and Heinesen, 2012; Michaud et al., 2016). Kuhn et al. (2009), Browning and Heinesen (2012) and Black et al. (2015) find that job loss has a positive effect on hospitalization and a negative effect on health. Conversely, Browning et al. (2006) as well as Salm (2009) do not find an effect of job loss on hospitalization or health.

What is still not clear in the literature is how job loss affects health and mortality. There is evidence that job loss affects mortality via reduced lifetime earnings (Sullivan and Von Wachter, 2009) and that job loss increases self-harm (Keefe et al., 2002; Browning and Heinesen, 2012), mental illness (Kuhn et al., 2009; Browning and Heinesen, 2012), traffic accidents (Browning and Heinesen, 2012), smoking (Black et al., 2015), drinking (Eliason and Storrie, 2009a; Deb et al., 2011; Browning and Heinesen, 2012), and that it induces having an unhealthier BMI (Deb et al., 2011). Our cause-specific mortality analysis shows that cerebrovascular diseases and smoking-related cancers are important drivers of the effect.

We use administrative employee-employer matched data from the Netherlands for the period 1999–2010. We estimate the effect of job loss on the probability of death within five years. There are a number of reasons why mortality is an outcome variable of core interest. First, length of life is directly relevant to the experienced welfare of individuals and their families. Second, mortality is an event which is distinctly and objectively observed in our data and does not raise issues of interpretation or subjectivity, unlike, say, self-reported health as available in surveys. As treatment, we use job loss due to firm closure with a sudden nature, based on an event that we are able to identify in the data: we select firms that have not experienced strong workforce decline in the years preceding closure. As all workers of closing firms lose their jobs, there is no selectivity of job loss within firms. Job loss due to this sudden type of firm closure is unlikely to suffer from anticipation of job loss, unlike wider definitions of closure-related job loss used in several other studies. US data suggest that job loss due to firm closure is relevant, as among those displaced from jobs held for at least three years, 37% or 1.2 million workers were displaced due to firm closure in the period 2013–2015 (Bureau of Labor Statistics, 2016).

The control group in our approach consists of observations on workers who stayed in their jobs in the year of observation. The treatment group consists of workers who were laid off in the year of observation specifically because of firm closure. For male workers with strong labor force attachment, we find that job loss due to firm closure increased the probability of death in the first five years after job loss by 0.60 percentage points or 34%. This result is in relative terms similar to what Eliason and Storrie (2009a) and Browning and Heinesen (2012) find, who do not control for firm-level worker characteristics. We find that without controlling for firm-level worker characteristics, job loss would have increased this probability by 0.83 percentage points or 46%. This suggests that not controlling for pre-existing differences in firm-level worker characteristics such as mortality rates between workers employed in closing firms and those employed in firms that continued operations indeed biases the estimate of the mortality effect of job loss upwards.[3]

The remainder of this paper is organized as follows. Section 2 provides background on the data and furnishes descriptives. Section 3 discusses the institutional setting. Section 4 explains the identification strategy and Section 5 presents the empirical results. Section 6 concludes.

2. Data

We have Dutch administrative micro panel data available for the period 1999–2010. The data are administered by Statistics Netherlands and cover the entire Dutch population. We have access to data on job and personal characteristics, mortality and hospital stays. These data can be linked through a personal identifier.[4] In addition, the data on job characteristics can be linked through a job and employer identifier. We measure labor force status, firm characteristics and lay-off in 2003–2005. We use data from previous years to construct variables such as pre-existing differences in firm-level worker mortality, and we use data from subsequent years to construct the probability of death within five years.

The personal characteristics file contains information on demographic characteristics such as marital status, number of children, country of birth, birth year and birth month. The job characteristics file provides information on all jobs any individual had been employed in. For every job, both start and end date, the industry code and the annual wage income are available. We deflate income to base year 2004. The job characteristics file also provides information on whether job terminations were due to firm bankruptcy or mass layoff and it enables us to define job loss due to firm closure. The mortality file contains information such as month, year and primary cause of death. The hospital stay file provides information for every hospital stay such as the start and end date of the spell, the reason for the stay and where the patient went after being discharged from the hospital.

For our analysis we make a baseline selection of observations, as we want to restrict ourselves to a group of workers for whom we may reasonably expect a mortality effect of job loss. In Section 5.4.3, we verify whether our result is robust to changing the various data selection criteria. In general, we find that our result is very robust. Our baseline sample is restricted to observations on male workers in the age category 45–59 who had stable employment relationships.[5] We define stable employment as having had continuous job tenure of at least five years with the present firm, while having had a real wage income of at least 20,000 euros in the preceding year.[6] We focus on older workers, because annual mortality rates of young workers are very low to begin with. As death is so

[3] The effect of job loss on mortality with controlling for firm-level worker characteristics is not statistically significantly different from the effect of job loss on mortality without controlling for firm-level worker characteristics at the ten percent level.

[4] The original file names are *SSB Banen (1999–2006), BEONTTAB (2003–2005), SSB Personen (2002–2005), Doodsoorzaken (1999–2010)* and *Landelijke Medische Registratie* (*LMR, 2002–2004*). Statistics Netherlands only provides data that are administered by governmental institutions. The data that are administered are not always available for the years we are interested in.

[5] We do not include women in our baseline sample, because we study the effect of job loss on mortality for older workers with strong labor force attachment. Strong labor force attachment is meant to make sure that stakes are high upon job loss. Since mortality is a low probability event for most working age individuals, we need large numbers of older workers to have sufficient statistical power for performing a sound analysis. Labor force participation of women was much lower than for men, while part-time work was much more frequent for women for the birth cohorts we study.

[6] As is customary, our register data do not contain information on hours worked. The income threshold of 20,000 euro corresponds to about 130 percent of the annual

rare among the young, if anything, we may expect to find mortality effects of job loss for older workers rather than for younger workers for the short time horizon we study. We only consider workers with stable employment relationships, because those with unstable jobs may be used to losing and switching jobs, or they may work so few hours that job loss may not be shocking for them.

Following Browning and Heinesen (2012), we exclude observations on workers employed in firms that had less than five workers.[7] We do so in order to remove self-employed and their employees from the sample. Those small firms may be inherently unstable. We also exclude observations on workers employed in large firms that had 400 workers or more, because large firms may be older, more stable and less likely to close, so they predominantly end up in the control group without having a counterpart in the treatment group.

We define job termination due to firm closure as job terminations in firms that do not appear in the dataset anymore during later years. We exclude observations on job departures other than layoffs due to firm closure, because workers departing their jobs for other reasons receive a different treatment than the one we are primarily interested in. We exclude observations on workers who were laid off due to firm closure if at least 40% of the workforce of the closing firm got employed in one particular firm within one year after firm closure.[8] We do so to rule out takeovers or restarts of firms after closure that are not separately coded in the data (also see Browning and Heinesen, 2012).

Lastly, closing firms may have laid off significant shares of their workforces prior to closure. This is a potential threat to the validity of our approach for various reasons. First, layoffs of co-workers and the threat of layoffs of workers may impose stress on workers and may possibly affect workers' health and health behavior. Second, prior firm-level layoffs may make workers expect that the firm closes or even goes bankrupt in the foreseeable future. Especially, relatively productive workers with good job prospects elsewhere may leave the firm and find alternative employment once they expect the firm to shut down (Henningsen and Hægeland, 2008; Schwerdt, 2011). Third, prior firm-level layoffs may be selective, as the least productive workers may be fired first when the firm experiences hard times (Pfann, 2006). Ill-health workers may be among the least productive workers. Whether the aggregate outflow of workers from the firm prior to closure is positively or negatively selected on workers' health is a priori unclear. We exclude observations on firms that reduced their workforce by at least 20% during the four years prior to the year of observation. This ensures that job loss due to firm closure comes relatively suddenly rather than after a period of prior layoffs.

Table 1 compares workers that stayed on the job (our control group) with workers who were laid off due to firm closure (our treatment group). It shows that workers who were laid off due to firm closure were on average very similar in terms of age, having been born in the Netherlands, marital status, number of children, hospitalization status (in $t-1$) and wage income (in $t-1$) to those who stayed on their jobs.[9] Workers who lost their jobs involuntarily in closing firms had on average lower job tenure than workers who stayed on their jobs. Job loss due to firm closure was relatively frequent in commercial services. In transportation and communication and construction, relatively few workers lost their jobs.

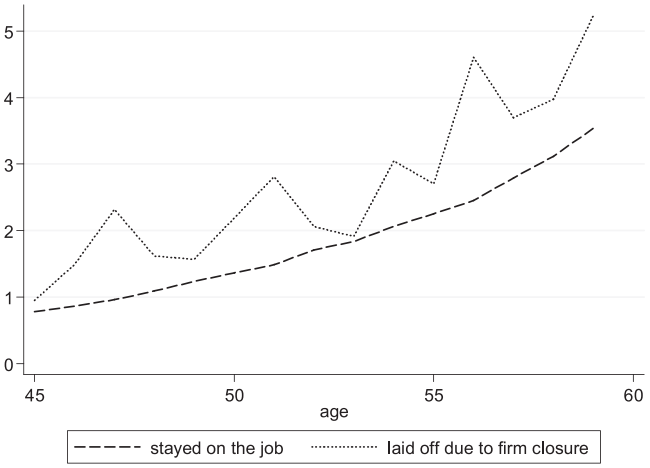

Fig. 1. Probability of death within five years for men (%), by labor force state.

Firm-level annual averages of worker characteristics are based on observations from all firms where any of the sample workers was working during the time of observation. Firm-level annual averages of most worker characteristics were similar for workers employed in closing firms and for workers employed in firms that continued operations. The percentage of workers having died during the preceding four years was on average higher for closing firms than for firms that continued operations. This suggests that workers employed in closing firms had on average poorer health than those employed in firms that did not close. Part of the explanation for these differences in firm-level worker mortality rates may be that workers in closing firms were on average slightly older than those in firms that continued operations.

Fig. 1 shows that the percentage of workers dying within five years was larger for workers who were laid off due to firm closure than for workers who stayed on their jobs.[10] The graph for laid-off workers is not as smooth as the graph for workers staying on their jobs. This is due to the small number of observations in the treatment group.

3. Institutional setting

The institutional setting within which job loss takes place may be very relevant when assessing the extent to which job loss translates into changes of (lifetime) earnings, that, in turn, can be directly relevant for mortality. For instance, using US data, Sullivan and Von Wachter (2009) find that the effect of job loss on mortality runs through (lifetime) earnings, but their measured effect may have turned out very different in labor market environments that featured different employment protection legislation, determination

minimum wage at full-time employment in 2004, making it less likely that the sample contains many part-time workers.

[7] As firms may consist of multiple plants between which jobs may be reallocated upon a shock, we select the sample on firm-level criteria.

[8] The 40 percent threshold is meant to strike a balance between two extremes. On the one hand, increasing the lower bound will retain more observations on workers employed in firms that are actually restarting or being acquired; their workforces may not be exposed to a severe shock, possibly biasing our result. On the other hand, reducing the lower bound to, say, 20 percent, would exclude a closing firm with five workers from the sample, if one of its workers got employed elsewhere within a year. Our threshold choice will be subject to sensitivity testing.

[9] Age (in years) is measured as per December 31st. Being born in the Netherlands is measured by a dummy variable that equals one if the worker and worker's parents were born in the Netherlands and zero otherwise. Hospitalization status (in $t-1$) is a dummy variable that is one if an individual was hospitalized during the previous year and zero otherwise. Wage income is deflated to 2004 and measured in thousands of euros. Job tenure is measured in years, as per January 1st. Marital status is a dummy variable that is one if an individual was married on January 1st and zero otherwise. Being married also includes having a registered partnership (a Dutch legal status effectively very similar to marriage). Cohabiting without being married or without having a registered partnership is not included in our definition of being married. Firm size is measured as the number of workers employed at the firm on January 1st.

[10] The five year mortality rate was significantly larger at the one percent level for workers who were laid off due to firm closure than for those workers who stayed on their jobs.

Table 1
Descriptive statistics.[a]

Workers who stayed in their jobs in the year of observation (control group)			Workers who lost their jobs due to firm closure in the year of observation (treatment group)		
Variable	Mean	Std. Dev.	Variable	Mean	Std. Dev.
Age	51.74	4.27	Age	52.33	4.39
Born in the Netherlands	0.90	0.30	Born in the Netherlands	0.89	0.32
Married	0.82	0.39	Married	0.81	0.39
Number of children	1.92	1.21	Number of children	1.89	1.20
Hospitalized [$t-1$]	0.0502	0.2184	Hospitalized [$t-1$]	0.0496	0.2170
Wage income [$t-1$]	44.65	23.60	Wage income [$t-1$]	45.57	24.80
Job tenure	16.18	8.02	Job tenure	14.12	7.99
Firm size	126.68	109.97	Firm size	121.71	103.47
Year	2004.00	0.81	Year	2004.04	0.84
Industry			Industry		
Agriculture	0.018	0.131	Agriculture	0.009	0.093
Asset Management	0.028	0.164	Asset Management	0.023	0.149
Banking/Insurance	0.013	0.115	Banking/Insurance	0.026	0.160
Catering	0.006	0.079	Catering	0.006	0.076
Commercial Services	0.073	0.261	Commercial Services	0.164	0.370
Construction	0.159	0.366	Construction	0.113	0.316
Education	0.074	0.261	Education	0.081	0.273
Health Care	0.015	0.121	Health Care	0.022	0.147
Manufacturing	0.283	0.450	Manufacturing	0.268	0.443
Other Care	0.057	0.231	Other Care	0.056	0.229
Public Sector	0.062	0.240	Public Sector	0.063	0.243
Retail	0.126	0.332	Retail	0.101	0.301
Transportation/ Communication	0.082	0.274	Transportation/ Communication	0.049	0.217
Temporary work	0.005	0.069	Temporary work	0.019	0.136
Firm-level worker characteristics			Firm-level worker characteristics		
% Died [$t-4$ until $t-1$]	0.88	1.41	% Died [$t-4$ until $t-1$]	1.05	1.68
Age	40.99	3.72	Age	41.92	4.28
Female	0.25	0.22	Female	0.26	0.23
Born in the Netherlands	0.87	0.11	Born in the Netherlands	0.86	0.13
Married	0.59	0.13	Married	0.60	0.13
Number of children	1.21	0.43	Number of children	1.21	0.44
Hospitalized [$t-1$]	0.0239	0.0237	Hospitalized [$t-1$]	0.0252	0.0253
Wage income [$t-1$]	32.66	14.99	Wage income [$t-1$]	34.94	15.26
Job tenure	8.08	3.27	Job tenure	8.49	3.97
N	840,886		N	8,394	

[a] Years of observation: 2003–2005, wage income deflated to 2004 and measured in 1000s of euros.

of pension benefits, eligibility criteria and length of unemployment benefits, or health insurance coverage, among others.

We do not expect strong short-term earning losses subsequent to job loss for the workers in our sample, because of generous and lengthy unemployment insurance benefits in the Netherlands during the period studied.[11] Substantial lifetime earnings losses may only be expected for workers experiencing unemployment for durations beyond the time horizon for which we consider mortality.

Fig. 2 shows that about 70% of the laid-off workers were employed during the calendar year after job loss. Fig. 3 shows that those employed during the calendar year after job loss had on average a similar wage income as during the calendar year before they lost their jobs. While such patterns can be consistent with laid-off workers experiencing only short unemployment spells with limited earnings losses, the unemployment scarring literature (e.g. Stevens, 1997; Eliason and Storrie, 2006) suggests that unemployment spells may result in life-time earnings losses even if short-term earnings effects are modest. Eliason and Storrie (2006) argue that strong tenure-based employment protection laws, such as the ones in the Netherlands, may make laid-off workers' labor market position more vulnerable to future shocks, possibly resulting in long-term earnings losses.[12]

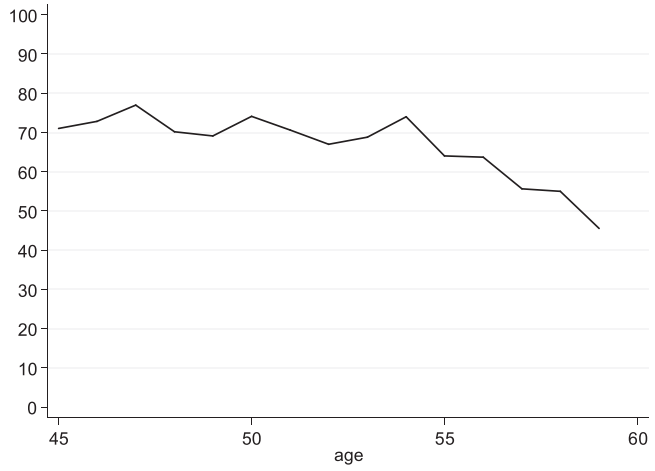

Fig. 2. Employment rate (%) in the calendar year following job loss.
This figure is based on observations on workers who lost their jobs in 2003 or 2004.

Accessibility of health care may principally be relevant for the effect of job loss on mortality as well. Reduced access to health insurance and health care due to the event of unemployment may trigger an effect of job loss on mortality. Especially if access to health care is limited, even modest untreated health issues may get out of control and result in some cases in death. In the Netherlands, there is mandatory health insurance membership for all residents,

[11] Some institutional detail on employment protection in the Netherlands can be found in the Appendix A.
[12] Endogeneity of unemployment duration, careers and earnings to workers' health prevents us from further studying the role of lifetime income.

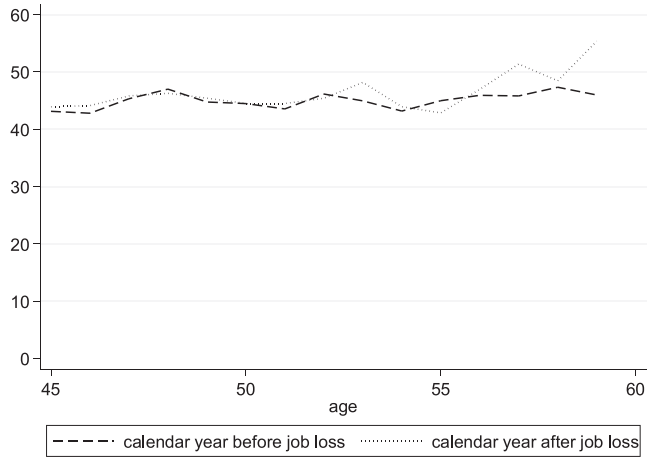

Fig. 3. Average wage income (in thousands of euros) for those who lost their jobs due to firm closure and found new jobs in the calendar year following job loss. Wage income is real wage income based on employment during the full calendar year. This figure is based on observations on workers who lost their jobs in 2003 or 2004.

independent of employment status. The government determines a minimum package of insurable risks that each health insurance contract must cover. Payments of health insurance premiums are subsidized for low income earners. Thus, basic access to health care is arranged for every resident. Given the universal access to health care and the comprehensive coverage, we hardly expect any effect of unemployment on mortality that can be attributed to reduced access to health care.[13]

4. Identification strategy

To delineate our identification strategy, it is helpful to introduce some notation. Let Y be a dummy variable for mortality within five years, the outcome of interest. For workers staying employed it equals one if the individual died within the five years after December 31st of the year of observation, and for job losers it equals one if the individual died within five years after the exact date of job loss. Y equals zero for all individuals staying alive. We are interested in the treatment effect of job loss on Y. Let D be a dummy variable indicating treatment. D is equal to one if an individual lost his job due to firm closure and zero otherwise.

Write the average treatment effect of job loss on mortality as

$$\alpha = \text{ATE} = \text{E}(Y_1|X) - \text{E}(Y_0|X) \tag{1}$$

with Y_1 the outcome under treatment ($D=1$) and Y_0 the outcome if untreated ($D=0$), and X observed control variables. The average treatment effect α can be expressed this way under the conditional mean independence assumption (2)[14]:

$$E(Y_0|\text{X, D}) = E(Y_0|X) \text{and } E(Y_1|\text{X, D}) = E(Y_1|\text{X}). \tag{2}$$

For the outcome Y we specify the linear probability model

$$Y = \beta_0 + D\alpha + X\beta_1 + \varepsilon \tag{3}$$

The expected value of the error term ε conditional on observables can be written as

$$E\left(\varepsilon_g|X\right) = Y_g - E\left(Y_g|X\right), g = 0, 1. \tag{4}$$

An unbiased estimate of the average treatment effect α can be obtained with (3), when making the additional assumption[15]

$$E(\varepsilon_0|X) = E(\varepsilon_1|X). \tag{5}$$

The zero conditional mean assumption (2) asserts that conditional on the control variables included in X, the treatment job loss due to firm closure was as good as randomly assigned. Within closing firms, all workers were dismissed, and thus worker job loss is independent of individual workers' health. However, it may well be that workers employed in closing firms differed from workers employed in firms that did not close. One concern is that closing firms may have hired a certain selection of workers that had relatively poor health and a relatively high probability of death within five years. We observe that closing firms actually had on average a higher fraction of workers dying during the four years preceding the year of closure than firms that did not close. The conditional independence assumption may be violated if we only control for individual worker characteristics such as age. We therefore control for pre-existing between-firm differences in health and mortality rates by including as control variables the firm-level fractions of workers having been hospitalized (in $t-1$), and of workers having died in any of the preceding four years, as well. The variables on individual worker characteristics included in X are a third-order polynomial in age, job tenure, a dummy variable for being born in the Netherlands, a dummy variable for being married, the number of children, a dummy variable for hospitalization (in $t-1$), wage income (in $t-1$), industry dummies and firm size.[16] X also includes time-varying firm-level fractions of workers being female, of workers being born in the Netherlands, and of workers being married. X further includes time-varying firm-level averages of workers' age, job tenure, number of children and wage income (in $t-1$).

The parameters of Eq. (3) are estimated using our data as pooled cross section. It is important to notice that workers in the control and treatment group are on average very similar in many dimensions, in particular demographics. This takes away the need to use a method such as propensity score matching to correct for observably unbalanced treatment/control samples. As many workers were observed in multiple years and all firms had multiple workers, we cluster standard errors both at the individual and firm level to account for serial correlation in the error terms. We perform sensitivity checks on our functional form specification in Section 5.4.1.

5. Results

5.1. Baseline estimate

Table 2 shows that job loss due to firm closure increased the probability of death within five years by 0.60 percentage points or 34%. This effect is significant at the one percent level. We discuss in Section 5.3 that our results on the effects of job loss on mortality on alternative time horizons are similar to those of Eliason and Storrie (2009a) and Browning and Heinesen (2012), who find that job loss increases the probability of death within four years by 44% and 36%, respectively. They use Swedish and Danish data on working age men, respectively.

[13] Further institutional details on health insurance can be found in the Appendix.
[14] Alternatively, the (stronger) conditional independence assumption can be imposed.
[15] This can be verified using (2) and (3) to write $E(Y_1 - Y_0|X) = \alpha + E(\varepsilon_1 - \varepsilon_0|X)$. Under (5), the latter term vanishes.
[16] We use the public sector as the base industry. The first, second and third order age terms are defined as $(age-45)$, $(age-45)^2$ *and* $(age-45)^3$. The industry dummies are dummies for each industry as classified by Nomenclature statistique des Activités économiques dans la Communauté Européenne (NACE) 1993 codes (Statistics Netherlands, 2004).

Table 2
Linear Probability Model (LPM) estimates for the probability of death within five years (in percentage points) (Ordinary Least Squares, OLS).[a]

Variable	Coef.	Std. Err.
Job loss	0.5968***	0.1722
(Age–45)	0.1513***	0.0304
$(Age–45)^2$	−0.0051	0.0058
$(Age–45)^3$	0.0007**	0.0003
Born in the Netherlands	0.1623	0.0719
Married	−0.7989***	0.0632
Number of children	−0.1121***	0.0191
Hospitalized [t − 1]	1.0750***	0.0866
Wage income [t − 1]	−0.0086***	0.0008
Job tenure	−0.0354***	0.0032
Firm size	0.0003	0.0002
Year	0.0150	0.0142
Industry		
Agriculture	0.1300	0.1530
Asset Management	−0.0133	0.1240
Banking/Insurance	0.3075*	0.1645
Catering	−0.1411	0.2628
Commercial Services	0.0952	0.1011
Construction	−0.1115	0.0952
Education	−0.0985	0.0970
Health Care	−0.1864	0.1449
Manufacturing	−0.0706	0.0875
Other Care	0.0505	0.1066
Retail	−0.0350	0.0922
Transportation/Communication	−0.0539	0.1047
Temporary work	−0.4936*	0.2965
Firm-level worker characteristics		
% Died [t − 4 until t − 1]	1.5024***	0.0256
Hospitalized [t − 1]	−1.4798**	0.6128
Age	−0.1437***	0.0075
Female	0.3911**	0.1830
Born in the Netherlands	−0.6322***	0.1767
Married	0.9028***	0.2255
Number of children	0.0769	0.0839
Wage income [t − 1]	0.0030***	0.0011
Job tenure	0.0672***	0.0081
Constant	−24.094	28.530
N	849,280	

[a] Public sector is the reference industry. Standard errors are clustered at the individual and firm level.
* Significant at the 10% level.
** Significant at the 5 % level.
*** Significant at the 1% level.

The coefficient estimates on most control variables show signs as we expect them to be. The probability of death within five years is positively related with age and being hospitalized and negatively related with wage income. The firm-level mortality rate shows a positive relation with individual mortality. This captures firm-level mortality effects from stress and work conditions, but also health-related hiring policy and other ways of sorting and selection of workers into firms. The firm-level annual averages of age and hospitalization status have negative coefficients (conditional on firm-level mortality).

5.2. Effects of job loss on cause-specific mortality

Specific causes of death may be related to working or being laid off. For instance, if job loss would induce high stress levels, the effect of job loss on mortality may run through diseases of the circulatory system, amongst others. We estimate the linear probability model in (3) using a dummy variable for death within five years due to a specific frequent cause as a dependent variable to get more insight in the mechanism through which job loss affects mortality.[17] We base the definition of causes of death on the 10th Revision of the International Statistical Classification of Diseases and Related Health Problems (ICD-10). The ICD is a health status classification system by the World Health Organization (WHO, 2010). The ICD groups causes of death into chapters, and we estimate the model separately per chapter. For the most frequent causes of death, cancer and diseases of the circulatory system, we also estimate the model for so-called blocks (subchapters). We estimate the model for intentional self-harm (suicide) as well, because this may be a relevant cause of death in the context of this paper. As job loss may induce workers to change their life style, we also estimate the model for alcohol-related mortality and mortality due to smoking-related cancers. There are no chapters or blocks that directly identify alcohol-related diseases or smoking-related cancers. Following Eliason and Storrie (2009a) for both of these groups of diseases and Browning and Heinesen (2012) for alcohol-related diseases only, we create corresponding categories. The category mortality due to smoking-related cancers consists of fatalities due to cancers of the respiratory system, among which lung cancer is the most frequent. According to Alberg and Samet (2003), 90% of the lung cancer cases can be attributed to active smoking. The category mortality due to alcohol-related diseases consists of fatalities that can directly be attributed to alcohol use.[18] We apply the Bonferroni correction for multiple hypothesis testing.

Table 3 shows that the coefficients on job loss are not significant at the ten percent level for mortality due to any of the specific death causes. This indicates that the effects of job loss on cause-specific mortality do not differ significantly across causes of death. Limited frequencies of cause-specific fatalities seem to be at least partly driving the lack of statistical significance. Small treatment group size contributes to coefficient estimates being imprecise and having in some cases values that appear unrealistically high or low. The Bonferroni correction being overly conservative in rejecting null hypotheses may be another factor explaining why the coefficient on job loss is not significant for any of the specific causes of death.

If we do not correct for multiple hypothesis testing, the coefficient estimate of job loss on the probability of death within five years is positive and significant at the five percent level for mortality due to diseases of the circulatory system and positive and significant at the ten percent level for mortality due to neoplasms (cancers). The effect on mortality due to neoplasms is driven by smoking-related cancers and the effect on mortality due to diseases of the circulatory system is driven by cerebrovascular diseases. Hypertension is the most important modifiable risk factor for cerebrovascular diseases.[19] Other risk factors include diabetes, obesity, alcohol use, smoking, lack of physical exercise, high cholesterol, high blood glucose levels and low fruit and vegetable intake (WHO, 2009).[20] Risk factors for hypertension include obesity, smoking, alcohol consumption, physical inactivity and stress (Kaplan and Nunes, 2003; Appel et al., 2006; Truelsen et al., 2006).

We find that job loss decreased the probability of death within five years due to external causes (excluding intentional self-harm). This effect is significant at the five percent level and may be explained by workers who stayed on their jobs dying in work-related accidents, or accidents that occur when commuting between home and work.

[17] We consider a cause to be frequent if there are at least 250 deaths recorded in our dataset.
[18] The causes of death classification with the corresponding ICD-10 codes is included in Table A1 of the Appendix A.
[19] Age is an important risk factor for these and most of the other diseases discussed as well, but is not modifiable.
[20] Risk factor indicates a factor that is correlated with the prevalence of a disease.

Table 3
LPM estimates for the effect of job loss on the probability of death within five years due to a specific cause of death (in percentage points).[a]

Cause of death	Coef.	Std. Err.	#Deaths
Alcohol-related diseases	0.0461	0.0291	113
Neoplasms	0.2377	0.1232	4,695
Malignant neoplasms of digestive organs	0.0885	0.0731	1,608
Malignant neoplasms of intrathoracic organs	0.0243	0.0662	1,424
Smoking-related cancer	0.0520	0.0264	36
Malignant neoplasms of urinary tract	−0.0301	0.0245	296
Malignant neoplasms, stated or presumed to be primary, of lymphoid, haematopoietic and related tissue	−0.0033	0.0298	309
Other malignant neoplasms	0.1062	0.0624	1,022
Diseases of the circulatory system	0.2453	0.1006	2,260
Ischaemic heart diseases	0.0882	0.0596	1,075
Cerebrovascular diseases	0.0875	0.0416	286
Other forms of heart disease	0.0696	0.0701	899
External causes of morbidity and mortality except intentional self-harm	−0.0389	0.0171	277
Intentional self-harm	0.0063	0.0291	271
Other diseases	0.1001	0.0671	1,126
Total	0.5968***	0.1722	8,741

[a] Each estimate is a coefficient estimate for job loss $\left(\hat{\alpha}\right)$ for the model as specified in (3) with the probability of death within five years due to a specific cause (in percentage points) as the dependent variable. The independent variables are job loss, $(age-45)$, $(age-45)^2$, $(age-45)^3$, being born in the Netherlands, marital status, number of children, hospitalization status (in t − 1), wage income (in t − 1), job tenure, firm size, industry dummies, year, firm-level annual percentage of workers dying during the preceding four years and firm-level annual averages on age, gender, being born in the Netherlands, marital status, number of children, hospitalization status (in t − 1), wage income (in t − 1) and job tenure. Standard errors are clustered at the individual and firm level. #Deaths are the number of unique deaths for each cause of death in our sample. Bonferroni correction for multiple hypothesis testing is applied. *Significant at the 10% level, **Significant at the 5% level.***Significant at the 1% level.

The positive effect of job loss on mortality due to cerebrovascular diseases and mortality due to diseases of the circulatory system in general is consistent with Browning and Heinesen (2012). They suggest that the effect of job loss on mortality due to diseases of the circulatory system runs through stress. Black et al. (2015) find that job loss affects health through smoking-related diseases. We find this as well.

5.3. Mortality effects of job loss by year since layoff

So far, we have focused on mortality within five years as the outcome variable. Estimating the model specified in (3) with mortality within fewer years as the outcome variable may provide insights on how long it takes for job loss to affect mortality. Table 4 shows that job loss has a positive 0.22 percentage point or 86% strong effect on mortality in the first year after job loss. The effect of job loss on mortality in the first two years after job loss is 0.34 percentage point or 61% and the effect of job loss on mortality in the first three years after job loss is 0.36 percentage point or 39%. The effect of job loss on mortality in the first four years after job loss is 0.55 percentage point or 41%. The relative sizes of these effects across time horizons are consistent with Browning and Heinesen (2012), who find that job loss increased mortality by 84% in the first year after displacement, 36% in the first four years after displacement and 17% in the first ten years after displacement. The relative size of the effect estimated by Eliason and Storrie (2009a), who find that job loss increased the probability of death within four years by 44%, is consistent with our result as well. On the mechanisms driving the short run effect, Browning and Heinesen (2012) and Eliason and Storrie (2009a) find that job loss increases the probability of death due to alcohol-related disease and suicides within one year. Browning and Heinesen (2012) find that job loss increases the probability of death due to circulatory disease and mental illness (as secondary cause of death) within one year as well.

5.4. Robustness checks

5.4.1. Robustness checks on functional form specification

We verify whether our baseline result is sensitive to functional form specification changes, and variations in the regressor set. We deviate from the baseline by leaving out one of the independent variables or one group of independent variables at a time. The effects of job loss on mortality are not significantly different from the baseline estimate for any of these alternative specifications. Table 5, variation a, shows that the effect of job loss on mortality when excluding firm-level worker characteristics is 0.83 percentage points or 46%. This is larger than the baseline estimate. Excluding individual firm-level worker characteristics generally does not affect our result much (variations b through j), except for excluding firm-level mortality which leads to the estimated parameter of interest to increase relative to the baseline (variation b). These results suggest that there is a bias in upward direction in the point estimate of the effect of job loss on mortality if we do not control for pre-existing differences in worker characteristics in general and pre-existing differences in worker mortality rates in particular.

Our baseline result is in general robust to leaving individual-level control variables or groups of such independent variables out of the model (variations k through t). The only exceptions relate to nonlinear age terms (variation k) and job tenure (variation q), giving estimates that are larger than the baseline.

We can add further worker characteristics through using a more fine-grained definition of marital status (including widowhood and divorce), its interaction with a dummy for having children, more detail on the origin of foreign-born individuals, and through using wage class dummies (instead of a continuous income measure). Doing so substantially increases the flexibility of the specification, even though we are unable to control for other dimensions such as education: the wage class dummies in particular can be useful, as they may partly pick up underlying effects of education, and experience. We also include appropriately defined firm-level worker averages. The coefficient estimate on job loss (variation u in the table) is very similar to the baseline estimate. Again, removing the firm-level worker averages from here (variation v) yields a very similar coefficient as in variation a. The exercise underscores the stability of our baseline estimate. More important, however, is that the difference in coefficient estimates on job loss between the specifications with and without controlling for firm-level worker averages is unaffected by the added flexibility.[21]

[21] The difference in job loss coefficient estimates between the variations u and v is 0.22 compared to a difference in coefficient estimates between the baseline and variation a of 0.23.

Table 4
LPM estimates for the probability of death within less than five years (in percentage points).[a]

The probability of death within	1 year	2 years	3 years	4 years	5 years
Coefficient estimate job loss $(\hat{\alpha})$	0.2229***	0.3435***	0.3645***	0.5471***	0.5968***
Standard error	0.0801	0.1073	0.1388	0.1522	0.1722
Relative effect (%)	85.8	60.9	39.4	41.2	33.5

[a] Each estimate is a coefficient estimate for job loss $(\hat{\alpha})$ for the model as specified in (3) with the probability of death within the number of years as specified in the table (in percentage points) as the dependent variable. The independent variables are job loss, $(age-45)$, $(age-45)^2$, $(age-45)^3$, being born in the Netherlands, marital status, number of children, hospitalization status (in t – 1), wage income (in t – 1), job tenure, firm size, industry dummies, year, firm-level annual percentage of workers dying during the preceding four years and firm-level annual averages on age, gender, being born in the Netherlands, marital status, number of children, hospitalization status (in t – 1), wage income (in t – 1) and job tenure. Standard errors are clustered at the individual and firm level. The relative effect is the coefficient estimate on job loss times 100 divided by the base fractions of workers who died within the relevant number of years for workers who stayed in their jobs. *Significant at the 10% level, **significant at the 5% level, *** significant at the 1% level.

Table 5
Robustness checks on functional form specification: LPM estimates for the probability of death within five years (in percentage points).[a]

Variation	Robustness check	Coef.	Std. Err.	Rel. effect (%)
	Baseline	0.5968***	0.1722	33.5
	Variations on firm-level variables			
a.	Do not control for any firm-level worker characteristic	0.8256***	0.1873	46.4
b.	Do not control for firm-level annual percentage of workers dying [t – 4 until t – 1]	0.7763***	0.1876	43.6
c.	Do not control for firm-level annual fraction of workers hospitalized [t – 1]	0.5966***	0.1722	33.5
d.	Do not control for firm-level annual average age	0.5649***	0.1739	31.7
e.	Do not control for firm-level annual fraction of female workers	0.5951***	0.1723	33.4
f.	Do not control for firm-level annual fraction of workers born in the Netherlands	0.6005***	0.1722	33.7
g.	Do not control for firm-level annual fraction of married workers	0.5943***	0.1723	33.4
h.	Do not control for firm-level annual average number of children	0.5966***	0.1722	33.5
i.	Do not control for firm-level annual average wage income [t – 1]	0.5990***	0.1721	33.6
j.	Do not control for firm-level annual average job tenure	0.6357***	0.1717	35.7
	Variations on worker-level variables			
k.	Do not control for age	0.7573***	0.1736	42.5
l.	Do not control for being born in the Netherlands	0.5976***	0.1722	33.6
m.	Do not control for marital status	0.5942***	0.1723	33.4
n.	Do not control for number of children	0.5979***	0.1723	33.6
o.	Do not control for hospitalization[t – 1]	0.5994***	0.1724	33.7
p.	Do not control for wage income[t – 1]	0.6081***	0.1718	34.2
q.	Do not control for job tenure	0.6816***	0.1720	38.3
r.	Do not control for firm size	0.5935***	0.1723	33.3
s.	Do not control for industry dummies	0.6063***	0.1721	34.0
t.	Do not control for year	0.5966***	0.1723	33.5
	Other variations			
u.	Control for: dummy variables for the worker being married, divorced or widowed, these dummies interacted with a dummy variable for having children; a dummy variable for the worker being from foreign but western descent (in addition to the dummy for being born in the Netherlands); dummy variables for wage income (at t – 1) categories[b]; and firm-level averages of the variables added here	0.5819***	0.1711	32.7
v.	Same as -u-, without controlling for any firm-level worker characteristic	0.8000***	0.1865	44.9
w.	Control for age fixed effects instead of nonlinear age effects	0.5966***	0.1723	33.5
	Other variations			
x.	Control for number of days hospitalized (at t – 1) instead of hospitalization[t – 1], also at the firm-level	0.6219***	0.1711	34.9
y.	Control for hospitalization[t – 2], hospitalization[t – 3], wage income[t – 2], wage income[t – 3], and firm-level averages of all those four variables	0.5963***	0.1722	33.5
z.	Control for natural logarithm of firm size instead of firm size	0.5958***	0.1722	33.5
aa.	Control for year fixed effects instead of linear year effects	0.5959***	0.1722	33.5

[a] Each estimate is a coefficient estimate for job loss $(\hat{\alpha})$ for the model as specified in (3) with the probability of death within five years (in percentage points) as the dependent variable. The independent variables are job loss, $(age-45)$, $(age-45)^2$, $(age-45)^3$, being born in the Netherlands, marital status, number of children, hospitalization status (in t – 1), wage income (in t – 1), job tenure, firm size, industry dummies, year, firm-level annual percentage of workers dying during the preceding four years and firm-level annual averages on age, gender, being born in the Netherlands, marital status, number of children, hospitalization status (in t – 1), wage income (in t – 1) and job tenure. Standard errors are clustered at the individual and firm level. *Significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

[b] The lagged wage income categories are 20–30 thousand euros, 30–40 thousand euros, 40–50 thousand euros, 50–60 thousand euros, 60–70 thousand euros, 70–80 thousand euros and more than 80 thousand euros.

If we include age dummies instead of nonlinear age terms as independent variables, the coefficient estimate is almost identical to the baseline estimate (variation w). Of interest for the present analysis is the definition of hospitalization, as our main health-related indicator. If we control for the lagged number of days hospitalized and its firm-level average rather than using the lagged hospitalization incidence indicator and its firm-level average, the main coefficient remains stable (variation x). The coefficient on job

loss is similar to the baseline coefficient estimate as well if we also include hospitalization and wage income at higher lags ($t-1$ through $t-3$), along with their firm-level worker averages (variation y). Our result is virtually unchanged as well if we include the natural logarithm of firm size instead of the level of firm size (variation z),[22] or if we use year dummies instead of a linear trend (variation aa).

5.4.2. Robustness checks on the type of job loss

Whereas we focus on job loss due to firm closure, there are other types of job loss treatments that can be applied to the estimation of the effect of job loss on mortality. These other types of job loss include job loss in firms experiencing large employment declines as applied in Sullivan and Von Wachter (2009), and job loss due to firm bankruptcy as studied in Keefe et al. (2002). The effects of job loss on mortality may differ across treatments and the workers receiving the treatments may differ in terms of characteristics. There may be heterogeneity of mortality effects of job loss across different groups of workers as well. Our data is unique in that it allows us to distinguish the three types of job loss and to compare their effects on mortality. The three types of job loss directly relate to each other, as job losses due to bankruptcy of firms are a subset of all job losses due to firm closures, which in turn are a subset of all departures from firms that were experiencing employment declines of at least 40% during the last year.

Table 6, variation a, shows that restricting the treatment group to job losses due to bankruptcy-caused firm closure, yields an insignificant coefficient estimate. We have applied propensity score weighting here, because of possible selectivity of worker populations of firms that experienced bankruptcy. The insignificance of the coefficient estimate on job loss is somewhat surprising, as there is no a priori reason to expect such difference with the baseline estimate. Extending the treatment group to job loss in firms whose workforce declined by at least 40%, shows an effect of job loss on mortality that is about one-third smaller than in the baseline model (variation b).[23]

5.4.3. Robustness checks on data selection criteria

We find an effect of job loss on mortality for a particular selection of observations that we believe represents a clean sample. We investigate whether our baseline result is sensitive to changing data selection criteria. We have not included observations on women in our baseline sample, because the main sample definition leaves few observations on women experiencing job loss. We do wish to explore the effect of adding observations on women, however. Table 7, variation a, shows that the coefficient estimate on job loss remains similar to the one for the baseline sample. We also estimate the model including a dummy for being female and its full interaction with all other independent variables (variation b). The coefficient estimate on job loss is almost identical to the baseline case. The coefficient estimate on the interaction term between job loss and the worker being female is not significant, however, indicating that the effect of job loss on mortality is similar for women and for men.

Because death is relatively rare among young individuals, our baseline selection only consists of observations on workers in the age category 45–59. Some other studies (such as Browning and Heinesen, 2012) use observations on all working age men, though. The mortality effect of job loss on a wider sample based on workers in the age category 21–59 is almost identical to the baseline estimate (variation c). In relative terms, the effect for workers in the age category 21–59 (61%) is much larger than the effect for the baseline sample (34%). This is due to the baseline mortality rate for the younger workers being much lower than for the older workers. If we restrict the sample to workers in the age category 21–44, the mortality effect is in absolute terms much smaller than the baseline effect (variation d), though it is in relative terms (61%) still larger than the effect for the baseline sample.[24] Hence, if we include workers in the age category 21–59 in our sample, the effect we find is strongly driven by the older workers.

The baseline excludes workers employed in firms with fewer than five and with more than 400 workers. We do so to keep our control group and treatment group comparable. Very large firms may rarely close, so that their workers end up in the control group only. Firms with less than five workers include small-scale self-employed; this group of firms can be expected to be relatively unstable. Therefore, workers employed at these firms may end up disproportionally often in the treatment group. Variation e shows that the effect of job loss on mortality for the extended sample including observations on workers employed in firms with less than five or more than 400 workers is substantially larger than the baseline estimate — owing to the firm size distribution being strongly skewed to the right.

Similar to Sullivan and Von Wachter (2009), the baseline excludes observations on workers with job tenures of less than five years to ensure that workers had stable employment relationships. The effect of job loss on mortality is no longer significant if we also include observations on workers with job tenures of at least one year in our sample (variation f). People with unstable jobs may be so used to job loss that job loss is not too much of a shock for them and that it does not subsequently kill them. Going the other way and excluding workers with job tenures shorter than ten years results in an effect that is larger than the baseline effect (variation g). Workers with long job tenures may have more firm-specific human capital than workers with short job tenures. This may make job loss lead to larger subsequent income losses for workers with long job tenures than for those with short job tenures. Larger income losses may lead to larger mortality effects of job loss, possibly also through increased stress levels.

The baseline model only selects workers with wage incomes of at least 20,000 euros to ensure strong labor force attachment. Lowering this threshold to 10,000 euros hardly affects the number of observations in our sample and has a negligible impact on the size of the effect (variation h). The small effect on the number of observations in our sample reflects that older male workers in their career jobs typically did not work only few hours. Conversely, excluding workers who earned less than 30,000 euros strongly reduces the number of observations. In this case, the effect of job loss on mortality is smaller than the baseline estimate (variation i). One potential explanation is that workers who earn relatively little are more likely to just make ends meet. Such workers may not be able to cover regular monthly bills anymore upon job loss. The resulting stress may then negatively affect their health and increase their probability of death within five years.

Our main sample does not include observations on job exits other than job loss due to firm closure. An alternative approach is to include in the control group job exits that were not due to firm closure. The effect of job loss on mortality estimated for the sam-

[22] We have tried various other specifications for firm size such as dummies for firm size groups as well. Estimates for the effect of job loss on mortality were similar to the baseline estimate for all these alternative specifications.

[23] The same criteria for selection of these observations apply as for the observations in the baseline dataset, except for the criterion on the treatment the worker received.

[24] Sullivan and Von Wachter (2009) find that the effect of job loss on mortality is smaller for older workers. We have verified whether this is also true in our case, adding the dummy variable for job loss interacted with age as an independent variable to the model as specified in (3). We find that the coefficient on job loss interacted with age is not significant at the ten percent level, however.

Table 6
LPM estimates for the probability of death within five years for alternative definitions of job loss (in percentage points).[a]

Variation	Definition of job loss	Coef.	Std. Err.	N treatment group	Rel. eff. (%)
	Job loss due to firm closure (baseline)	0.5968***	0.1722	8,394	33.5
a.	Job loss due to firm bankruptcy	−0.2411	0.6279	1,552	−13.5
b.	Job loss due to a firm-level employment declines of at least 40%	0.3797***	0.1460	10,693	21.3

[a] Each estimate is a coefficient estimate for job loss $\left(\hat{\alpha}\right)$ for the model as specified in (3) with the probability of death within five years (in percentage points) as the dependent variable. The independent variables are job loss, $(age-45)$, $(age-45)^2$, $(age-45)^3$, being born in the Netherlands, marital status, number of children, hospitalization status (in t − 1), wage income (in t − 1), job tenure, firm size, industry dummies, year, firm-level annual percentage of workers dying during the preceding four years and firm-level annual averages on age, gender, being born in the Netherlands, marital status, number of children, hospitalization status (in t − 1), wage income (in t − 1) and job tenure. The control groups are the same in variations a and b as in the baseline case. Variation a is estimated using propensity score weighting, because workers laid off due to firm bankruptcy are not comparable to those in the control group. Standard errors are clustered at the individual and firm level for variation b and the baseline estimate and clustered at the individual level for variation a. The relative effect is the coefficient estimate on job loss times 100 divided by the base fractions of workers who died within five years. * Significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table 7
Robustness checks on data selection: LPM estimates for the probability of death within five years (in percentage points).[a]

Variation	Robustness check	Coef.	Std. Err.	N
	Baseline	0.5968***	0.1722	849,280
a.	Sample including women	0.5584***	0.1444	1,075,559
b.	Sample including women; alternative specification[b]	0.5900***	0.1717	1,075,559
	Coefficient dummy job loss*dummy female	−0.1368	0.3129	
c.	Extended to workers in the age category 21–59	0.5983***	0.0927	1,914,500
d.	Only workers in the age category 21–44	0.2096**	0.0871	1,065,220
e.	Incl. workers employed in firms of any size	0.9252***	0.1537	2,113,677
f.	Incl. workers with job tenures of at least 1 year	0.1487	0.1248	970,872
g.	Excl. workers with job tenure less than 10 years	0.7450***	0.2430	631,466
h.	Incl. workers with wage income[t − 1] of at least 10,000 euros	0.6031***	0.1692	872,090
i.	Excl. workers with wage income[t − 1] lower than 30,000 euros	0.5031***	0.1814	702,327
j.	Incl. job exits not due to firm closure (included in control group)	0.5606***	0.1717	881,999
k.	Incl. firms with layoffs prior to the year of observation	0.5092***	0.1629	968,772
l.	Incl. cases of job loss due to firm closure if at least 40% of the workers employed in a closing firm got employed in one particular firm within one year after firm closure	0.5813***	0.1619	850,301

[a] Each estimate is a coefficient estimate for job loss $\left(\hat{\alpha}\right)$ for the model as specified in (3) with the probability of death within five years (in percentage points) as the dependent variable. The independent variables are job loss, $(age-45)$, $(age-45)^2$, $(age-45)^3$, being born in the Netherlands, marital status, number of children, hospitalization status (in t − 1), wage income (in t − 1), job tenure, firm size, industry dummies, year, firm-level annual percentage of workers dying during the preceding four years and firm-level annual averages on age, gender, being born in the Netherlands, marital status, number of children, hospitalization status (in t − 1), wage income (in t − 1) and job tenure. Standard errors are clustered at the individual and firm level. * Significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.
[b] The model in variation b also includes a dummy variable for being female and its interaction with all other independent variables.

ple including observations on workers leaving their jobs for other reasons than firm closure is almost identical to the effect of job loss on mortality estimated for the baseline sample (variation j). This is not surprising, as the additional number of observations is small compared to the total number of observations in the control group.

We exclude observations on workers employed at firms experiencing large employment declines prior to closure to minimize the risk that job loss is endogenous to mortality. As a robustness check, we estimate the effect of job loss on mortality for a sample that consists of observations on workers employed in firms that experienced any employment reductions in the years preceding closure. We find that this effect is smaller than the baseline effect (variation k). Workers employed in firms that experienced prior mass layoffs may have become better able to handle layoff-related stress by their prior experiences. This may have made the effect of job loss on mortality smaller for these workers.

The baseline sample excludes observations on workers who were laid off due to firm closure if at least 40% of the workforce got employed in one particular firm within one year after closure. We select our sample so as to rule out that closing firms may have restarted or may have been taken over by another firm, effectively keeping workers in employment. Estimating the effect of job loss on mortality for the dataset that includes these observations provides an effect that is almost identical to the baseline estimate (variation l).

6. Conclusions

We study the effect of job loss on mortality. As ill-health workers may be more likely to lose their jobs than healthier workers, simply regressing job loss on mortality will result in a coefficient estimate on job loss that is biased upwards. Studies in the literature attempt to avoid such endogeneity bias by using job loss due to firm closure (Eliason and Storrie, 2009a; Browning and Heinesen, 2012; Michaud et al., 2016) and job loss in firms experiencing large firm-level employment declines (Sullivan and Von Wachter, 2009) as treatments to estimate mortality effects of job loss. These treatments may still not be free of endogeneity bias, however. We actually find that firm-level mortality rates during the four preceding years were higher in closing firms than in firms that survived. These pre-existing differences in firm-level worker mortality rates may exist due to selective hiring or due to non-random worker outflow prior to firm closure or downsizing. We control for pre-existing differences in firm-level worker health and mortality rates and other firm-level worker characteristics. To our knowledge, we are the first paper in the literature to do so.

We find that job loss due to firm closure increased the probability of death within five years by 0.60 percentage point, or 34%. This is similar to the findings of Eliason and Storrie (2009a) and Browning and Heinesen (2012). We find that job loss increased the probability of death within five years by 0.83 percentage point or

46% if we do not control for firm-level worker characteristics. This difference in estimated effects, albeit statistically insignificant, suggests that the coefficient estimate of job loss on mortality is biased upward if we do not control for firm-level worker characteristics.

Taking a closer look at the mechanism driving the effect of job loss on mortality, we find that job loss due to firm closure may arguably run through stress, as cause-specific mortality analysis shows that (acute) diseases of the circulatory system are important drivers of the effect. The strong effect of job loss on mortality in the first year after job loss is consistent with an effect running through stress and (acute) diseases of the circulatory system. Eliason and Storrie (2009a) and Browning and Heinesen (2012) likewise find that diseases of the circulatory system are important drivers of the effect of job loss on mortality. Changes in lifestyle seem to be relevant too, because smoking-related cancers account for part of the effect of job loss on mortality. This is consistent with Black et al. (2015), who find that job loss negatively affects health through smoking-related diseases.

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Appendix A. Employment protection in the Netherlands

25. *Employment protection in the Netherlands*

Laid-off workers with employment tenures as the ones in our dataset were eligible for unemployment insurance (UI) benefits of up to five years, irrespective of whether the firm they were employed in closed down.[25] The potential UI benefit duration depended positively on the employment tenure of the worker.[26] The replacement rate was 70% of the final wage (Dutch Government, 2015a), up to a ceiling of 168 euro per day (44,000 euros per year; values for 2004).[27] So, even if workers were not re-employed soon after job loss, the effects of job loss on net income on time horizons shorter than five years were arguably modest for workers with income levels below 44,000 euros per year but larger for workers with income levels above this threshold.

In addition to UI benefits, laid-off workers may have received severance pay. Workers who were laid off by firms that closed down but did not go bankrupt received severance pay from their employer in certain cases. There were two layoff routes for workers employed in this type of closing firms: via the court or via the Employee Insurance Agency (Dutch Government, 2015b,c). Layoffs via the former route typically resulted in severance pay for the laid-off workers. The levels of these compensations depended strongly and positively on age, job tenure and wage income. Compensations for workers aged 45 with job tenure of five years, i.e. the youngest workers with the lowest job tenure in our dataset, typically amounted to about half an annual wage. In the latter case employers had to seek permission by the Employee Insurance Agency to lay off workers. If permission was granted, employers could typically lay off workers without giving them any compensation. Workers may in this case have gone to court to seek compensations from their (former) employers.

Workers did typically not receive any severance pay for job loss due to firm bankruptcy (Berntsen and Mulder, 2014; Van Riet, 2014),[28] except for two, rather uncommon, cases. First, if some inventory remained after the trustee and preferred creditors were paid. Second, if a board member of the employer inappropriately extracted financial wealth from the firm prior to bankruptcy and he or she was held liable for this (Dutch Government, 2015d).

The oldest workers in our dataset may have used UI as an alternative retirement path.[29] The social security eligibility age in the Netherlands was 65 during the period studied. Early retirement pensions of occupational pension funds generally offered benefits as of age 60, 61 or 62 in our dataset, depending on the pension fund. They typically required workers to have been employed continuously for a certain number of years prior to early retirement. As workers in our dataset did not reach the age of 60 yet, they were not eligible to retire early after job loss without getting re-employed first. However, UI benefits may have provided the opportunity to

Table A1
Cause of death classification.

Cause of death	ICD−10 codes[a]
Alcohol-related diseases	F10, G31.2, G62.1, K29.2, K70, K85.2, K86.0, T51, X45, Y15
Neoplasms	C00–C97, D00–D48
Malignant neoplasms of digestive organs	C15–C26
Malignant neoplasms of intrathoracic organs	C37–C38, C39.9
Smoking-related cancer	C30–C34, C39.0, C39.8
Malignant neoplasms of urinary tract	C64–C68
Malignant neoplasms, stated or presumed to be primary, of lymphoid, haematopoietic and related tissue	C81–C96
Other malignant neoplasms	C00–C14, C40–C63, C68–C80, C79, D00–D48
Diseases of the circulatory system	I00–I99
Ischaemic heart diseases	I20–I25
Cerebrovascular diseases	I60–I69
Other forms of heart disease	I00–I19, I26–I59, I70–I99
Symptoms, signs and abnormal clinical and laboratory findings, not elsewhere classified	R00–R99
External causes of morbidity and mortality except intentional self-harm	V00–V99, W00–W99, X00-59, X86–X98
Intentional self-harm	X60-X85
Other diseases	A00–A99, B00–B99, D50–D89, E00–E90, F00–F99, G00–G99, H00–H95, J00–J99, K00–K93, L00–L99, M00–M99, N00–N99, O00–O99, P00–P96, Q00–Q99, R00–R99, S00–S99, T00–T99, U00–U85, Z00–Z99

[a] More information on the ICD-10 can be found on http://apps.who.int/classifications/icd10/browse/2015/en.

[25] Eligibility was conditional on, for instance, actively looking for a job.

[26] The data of Statistics Netherlands do not go back far enough in the past to determine the maximum duration for unemployment benefit per individual.

[27] Workers did not build up occupational pension capital during unemployment.

[28] Workers may have asked the judge to get severance pay in case of firm bankruptcy. This severance pay was paid out of what was left of the inventory after the trustee and preferred creditors had been paid. However, as there was typically nothing left of the inventory, chances to get severance pay by making a request to the judge were very low.

[29] Chan and Stevens (2001) find that job loss among older workers has strong and lasting effects on employment rates. Tatsiramos (2010) finds that older workers who are displaced in countries with relatively more generous UI provisions have relatively lower re-employment rates.

close part of the gap in earnings between the time of job loss and reaching the normal retirement age, especially for the oldest workers in our sample.

Job loss may have affected lifetime income through lower pension benefits as well. Occupational pensions are predominantly of the defined benefit type, with replacement rates depending on average career wage income and the number of years individuals have contributed to the pension scheme. Non-working individuals not receiving UI benefits do not contribute to the pension scheme in principle. They typically contribute less to the pension scheme if they do receive UI benefits. Due to contributing less or nothing during spells of unemployment, individuals losing their jobs also lose some of their pension benefits.

30. The Dutch health insurance system

The Dutch health insurance system features mandatory participation and subsidization of premiums (contributions, fees). The system underwent important changes in 2006. Before that year, there was mandatory health insurance membership for people with income levels below a threshold.[30] This national insurance had a large coverage of medical treatments. Individuals with higher incomes could take private health insurance or health insurance offered through their employers. As of 2006, there is a compulsory basic health insurance for all residents. Basic health insurance covers health expenditures for a large variety of treatments, specified by the government. There was a no-claim bonus in 2006 and 2007 and there is a mandatory minimum deductible as of 2008.[31] People with low income levels get a monthly benefit that helps them pay their basic health insurance premiums. Over and above the mandatory basic coverage, people can buy supplementary health insurance for, e.g., vision care and dental care. Long-term care is not covered by the basic health insurance, but financed through a separate universal system to which all workers pay premiums.

32. The Dutch pension system[32]

The Dutch pension system rests on three pillars (Bovenberg and Meijdam, 2001). The first pillar is the public old-age pension system, financed on a pay-as-you-go basis. Contributions stem from workers and employers. All residents registered in the Netherlands accrue public old-age pension rights. Public old-age pension benefits are flat. For couples, they equal the minimum wage. Singles receive 70% of the minimum wage. For every year between the ages 15 and 65 that an individual does not reside in the Netherlands, public old-age benefits are cut by two percentage points. The second pillar consists of occupational pensions (including company-specific funds of large firms, and industry-wide funds covering all occupations in an industry). These are funded pensions and are generally managed on the sector level. The third pillar consists of private provisions. Private provisions include, amongst others, annuities or life insurance policies.

About 90% of all workers participate in an occupational pension plan. Occupational pension schemes receive contributions from workers and employers. Workers who participate in a pension plan pay contributions over the difference between their wage and a nominal threshold called the “franchise”. The franchise is about 143% of the public-old age pension benefit level. In this way, first-pillar old age pensions and second-pillar occupational pensions are integrated and jointly achieve before-tax replacement rates of 70% of the average pay for the cohorts we study. As every firm or sector has its own pension plan and pension conditions, there is a large heterogeneity among occupational pensions. For the time period studied, early retirement pensions, embedded in the occupational pension system, were widespread, owing to tax incentives.[33] There was a large heterogeneity in early retirement arrangements across sectors as well.

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[30] The income threshold gradually increased across years and was 33,000 euros in 2005.

[31] The no-claim bonus for basic health insurance was 255 euros in 2006 and 2007. The deductible for basic health insurance was 150 euros in 2008 and gradually increased afterwards.

[32] This section is obtained from Bloemen et al. (2017).

[33] The so-called fiscal facilitation of the early retirement contributions implied that the early retirement benefits were taxed, and that the early retirement contributions paid by workers and employers were exempted from taxation. As effectively less tax was paid, the fiscal facilitation made early retirement very attractive for both eligible workers and employers.

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